Review Comments: Species Report

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The draft fisher species report ("report") represents the most comprehensive review of the literature on western fishers ever compiled. The report will be the definitive source of information about fishers in the Pacific states for years to come. Regulatory decisions aside, the compilation of information is – in and of itself -- a significant contribution to fisher conservation. Moreover, as part of assigning scope and severity values to threats in each bioregion, the report includes the first attempts to understand the magnitude of these threats using quantitative measures, when possible. This includes, for example, developing "coefficients of management activity" (pg. 87), new climate change analyses (pg. 72-76), estimating scope for AR effects in California (pg. 166), and specific fire analysis (re: Rim fire, pg. 67). Thus, the report represents not only an assessment of existing knowledge but includes new analyses that can be considered new and original science. I have reviewed similar reports as part of the ESA listing process for other species (i.e., wolverine, jaguarundi and ocelot), and the current fisher report is by far the most comprehensive. It was difficult to identify any peer-reviewed published work or relevant published work on fishers that was not referenced (a few are included among my comments, below). And, relevant information from sources as diverse as congressional hearings (eg. Regarding ARs) and obscure (e.g., Reno et al. 2008) and early literature (e.g., Buck 1982, Jones 1991) were well represented. I also appreciate that the authors drew on references from the eastern portion of the fisher's range, or from the marten literature, only when it was absolutely necessary. We are fortunate to have a wealth of new fisher research in the West and a number of existing recent reviews of that literature to draw from and the report exploits this wealth. Finally, the copy editing was superb. The documents were well-written and well-edited and, despite the report's length, they were easy to read and comprehend.

Importantly, where the data are absent or ambiguous or the inferences not clear, the authors appear to apply a precautionary position and recognize that small and otherwise vulnerable populations of fishers may be at risk. The author's assumptions, in these cases, appear to err on the side of caution. A few examples include: (1) assuming that fisher's home ranges will shift and fitness will be lower in response to high severity fires (pg. 63), (2) scope and severity estimates for vegetation management on Federal lands that do not account for future habitat growth (pg. 95) and (3) comments on snag retention that note that many snags don't meet minimum sizes for use by fishers and that short rotations on private timber land rarely allow for the development of large snags and logs (pg. 143). In each case, although there are rarely data to cite, general inferences from the fisher habitat ecology literature, and conventional wisdom, are used to describe the potential negative effects. In some cases, assumptions are stated outright so that the reader can decide how to interpret the circumstance. In general, I agree with how the authors handled these areas of uncertainty.

Note: Unless otherwise noted in the sections below, I am in agreement with the analytical methods and the conclusions stated in the rule and the report. There are myriad issues that have an underlying

scientific basis. Most of these I do not address or contest; in fact I agree with the vast majority of them. I will not, however, note my agreement with these various issues except to acknowledge it here.

General Topics:

1. **Trade-off between negative and indirect positive benefits of fuels treatments.** The proposed rule requests comment on the effect of fire and fuel treatments on fisher habitat (12a on pg. 60421). The report covers very comprehensively the potential negative effects of fire on fisher habitat, including the increase in fire scope and severity expected under future climate change. It also focuses attention on the potential negative effects of fuels treatments on fisher habitat (pg. 85 – 95) creating a "coefficient of vegetation management" which, if I understand it, implies that the higher the coefficient the more risk to fisher habitat in the ensuing 40 years. It appears that the "percent baseline…affected" (pg. 89) and "% timberland harvested" (pg. 91) assumes that these acres all are degraded, when in fact – especially on federal lands – some of these acres are treated with either mechanical means or Rx fire (or both) to reduce the loss of habitat due to wild fire. I didn't see an analysis that attempts to evaluate the trade-offs between the negative direct effects of fuels treatments on fisher habitat and the indirect benefits, in terms of reduced severity of wildfire. Some modeling (Scheller et al. 2011) suggests that the indirect benefits of fuels treatments can be positive and some field work demonstrates that fishers appear to tolerate the rate and extent of treatments that fire ecologists suggest are necessary to reduce fire severity and fire spread rates (Zielinski et al. 2013). It is my scientific opinion that, on balance, widespread and aggressive fuels treatments pose a significant risk to fishers but there is a trade-off between their costs and benefits that, despite their importance, do not appear to be adequately represented in the report.

2. **The gap in fisher distribution in California.**
   The report represents a nuanced and, in my opinion, appropriate consideration of the historical and current gap in the fisher distribution in the Sierra Nevada. This is a critical element of the review because of the implications. If one assumes the gap was large prior to human influence, then the current distribution may not be interpreted as very different from the historical distribution. If, on the other hand, one views the historical gap as small or nonexistent – which seems to be the way the report leans (e.g., see Fig. 5, Fig. 18) – then the current distribution is significantly smaller than the historical distribution. The evidence pits sophisticated molecular genetic data against old fashioned accounts by naturalists and trappers; bodies of evidence that are difficult to reconcile. I think the document puts the matter in proper perspective. The authors may wish to, however, consult the recent status review by the state of California (Dept. of Fish and Wildlife) that includes a very comprehensive summary of the collection of miscellaneous historical information that supports the assumption that the fisher distribution in the Sierra was continuous during the 19th and early 20th centuries (see 1 Oct 2014 draft: pg. 12-14).

I should also mention, in regard to the influence of the "gap" in California on fisher ecology, that Lori Campbell -- in her unpublished and frequently overlooked PhD dissertation (cited below) -- conducted an analysis to compare the environmental features associated with the places where fishers occur in the Sierra and the places that they no longer occur (the gap). She found that the occupied area had more and larger trees (conifers and hardwoods), steeper slopes, more shrub cover and fewer roads than the unoccupied area (see her Table 5 and Fig. 10). This suggests that the absence of fishers may be caused by, or their recolonization slowed by, all or some of these factors.

Campbell, L.A. 2004. Distribution and habitat associations of mammalian carnivores in the central and southern Sierra Nevada. PhD dissertation, UC Davis, Davis, CA.

3. **Absence of section on community ecology/interspecific interactions**.

I was surprised by the absence of a section on community ecology, especially one dealing with potential competitive interactions with martens and other forest carnivores. Bill Kroon's papers speak to the environmental circumstances favoring marten or fishers. Lori Campbell's dissertation focuses on this subject, but is not cited in the report (see citation, above). For example, she found a negative association between gray fox/spotted skunk and fishers in the Sierra and suggested that "*elevated densities of generalist species may hinder the return of fishers to portions of their range*…" In addition, the change in forests expected as a result of climate change may tip the balance in competitive interactions among fishers and other carnivores. Finally, there is a new paper by La Point et al. (2014) on trophic cascades and mesopredator release in fisher communities that is also relevant (see citation below). In aggregate, I think these papers (and perhaps others?) would make for a relevant section on "community interactions" which may lead to some synergistic interactions with existing stressors.

LaPoint S.D. 2014. Mesopredator release facilitates range expansion in fisher. Animal Conservation doi:10.1111/acv.12138

4. **Inadequate consideration of rest structure reuse rates.**

Fisher's reuse of resting structures is quite low in most studies. Less than 10% of rest structures are found to be reused during any particular study (e.g. Zielinski et al. 2004). I note this fact mentioned here and there in the report, but is has important implications in habitat management and in assigning risk from vegetation management. It also is a distinction that comes into play when comparing the habitat needs of spotted owls and fishers. Owls roost in a variety of locations in the forest and typically use a single large structure (as a nest site) whereas the forest needs to provide perhaps hundreds of large woody structures (i.e., old live trees or snags) for each fisher. Managing for this need, as well as the need for the connectivity required by a terrestrial mammal, make the fisher's needs harder to accommodate than those of the spotted owl. I'm not arguing that owl habitat may not be a good proxy for fisher habitat, but that more should be made of the large number of distinct resting structures that are required by fishers compared to, say, forest-dependent birds such as spotted owls or murrelets.

5. **No summation of the scope and severity effects?**
   I thought that the purpose of assigning scope and severity to each stressor was so that they could be combined in some fashion to represent "impact". Yet, I see no summary table that ranks stressors by the combined influence of their scope and severity, only Tables 27-34 that reiterate the scopes and severities. What am I missing?

Specific Comments:

1. Pg. 8-9. Note that the official scientific names of North American mammals has been updated to include *Pekania pennanti* (Bradley et al. 2014).

   Bradley et al. 2014. Revised checklist of North American mammals north of Mexico, 2014. Occ. Papers of the Museum of Texas Tech. No. 327. 28 pp.

2. Pg. 10, first sentence of "Survivorship" section. Buskirk et al. (2012; their chapter in *Martes* symposium proceedings book) also includes analyses that support the importance of adult female survival in predicting population performance.
3. Pg. 11. Keith Slauson has reported, at a recent TWS meeting, an inverse relationship between the size of fisher home range and the proportion of the diet that is composed of prey > 200 g in size. This is currently unpublished, but potentially important new information.
   Slauson, K.M. and W.J. Zielinski. 2012. Effects of diet composition on home range size by fishers in California. Abstract of talk presented at TWS Western Section meeting.

4. Fig. 4. This is impressive, but it would be helpful to also include a figure that identifies only the 456 points that are verified (in recorded history). None of the figures later in the section (Figs. 7, 8, 9) appear to include all 456 points, from 1896 – present.
5. Pg. 35. I don't understand the purpose or the point of this paragraph. So there were a few fishers in the northern Rockies prior to the various reintroductions there. Isn't augmenting a population that was so small so as to be virtually undetectable as justifiable as reintroducing a population after its extirpation? One of the key points that is lost when the conservation genetics considerations take front-center is that a few remaining individuals that may have a unique genetic composition are not playing the important role that a healthy fisher population – augmented by translocations – contributes to the ecosystem.
6. Pg. 39, 3rd paragraph. Regarding the analytical process used to relate the fisher survey results to predicted habitat, something seems circular here. Weren't the fisher survey data used to build the habitat model in the first place? If so, wouldn't you expect that hexagons with high-value modeled habitat to have a relatively high number of surveys with detections (47% in this case, or 78% when corrected for detectability). Does the fact that they do not, only emphasize your point in the following paragraph: that there is considerable unoccupied habitat in the NCSO population? Or, is it the opposite? Hard to follow the logic here. Interesting analysis, but not enough detail here for me to fully evaluate. On the face of it, using spatially heterogeneous

survey data to assign occupancy to hexagons seems rife with assumptions that are difficult to defend.

7. Pg. 57, "Loss of late-successional forests from past activities…". I can't emphasize enough how important this section is. That much of the prime fisher habitat has already been lost from places like Washington and Oregon, is very relevant to the management of forests and fishers into the future. Without this cumulative look at loss of habitat, we experience the "shifting baseline syndrome" (Pauly 1995) and plan only to minimize the loss of a fraction of what is remaining, rather than to institute long-term planning to restore habitat conditions that occurred prior to widespread timber harvest. One of the most important sentences in the report is at the end of the second paragraph: "In western Washington and Oregon, modern estimates suggest that 82-87% of the late-successional forests present at the time of settlement have now been logged (Booth 1991)."
8. Pg. 89, Table 11. It is hard to believe that only "1 acre" of nesting/roosting habitat was removed or downgraded in the California Cascades and the California Coast over the 7-year period from 2006-2013. Are these typos?
9. Pg. 97. Timing, scope and severity for human development. Stated here, near the top, is: "Within much of the analysis area, human development is generally to be considered to be of relatively low concern for fishers and occurs at relatively small spatial scales in forested landscapes (Naney et al. 2012)". I wonder whether this accounts for the additional impact that occurs when the roads and human infrastructure is considered an asset to protect from wildfire? In many exurban locations the state or feds establish "wildland urban interface" zones (or something similar) where aggressive thinning and overstory removal are implemented to address fuels concerns. Thus, human development acts synergistically with vegetation management (in this case, intensive fuels treatments) to magnify the spatial impacts on fisher habitat. I did not notice where this interaction was considered.
10. Pg. 108. The first sentence under "Summary of effects of habitat stressors" states that "..habitat loss, modification, and fragmentation appear to be significant stressors to fishers." The document provides abundant evidence of the <u>loss</u> and <u>modification</u> of habitat, but does not appear to analyze how habitat has been <u>fragmented</u>. Thus, I don't find evidence in the document for the increase in fragmentation, which would be measured using some form of landscape metric (e.g. a comparison of patch size distribution over time, change in inter-patch distances). This may be academic, since given all the loss of habitat there certainly has been fragmentation as well, but I note here an exception to the general practice of supporting statements with either references to the literature or via original analysis.
11. Pg. 119-120, Re: Federal Regulation. In my experience NEPA and the Forest Service's Sensitive Species program do not provide much specific protection for fishers. Not only does NEPA not require or guide potential mitigation for impacts to fishers, but many analyses conclude that projects may affect individuals but not populations. However, when one views the effects on individuals in aggregate, the totality of vegetation management activities may indeed affect populations, a cumulative risk that is not accounted for in NEPA analyses. And, although I agree *in principle* that projects "must be analyzed for their potential effect on sensitive species" (pg. 120), this analysis is typically very superficial and does not provide mitigations for negative

effects on habitat. Thus, although the authors intimate that NEPA and the Sensitive Species program may not provide a good safety net for fishers, I would argue that the net is even less effective than described in the report.

12. Pg. 124, Re: the SNFPA. Great point regarding the overlap of the fisher conservation area with WUI and tribal fuel treatment areas. This does, potentially, diminish the benefits of the conservation area. However, not mentioned here are the potential indirect benefits of fuels treatments on fisher habitat, by reducing the size and severity of wild fire (see Scheller et al. 2011, Zielinski et al. 2013).
13. Pg. 124, Re: the Fisher Analysis Suitability Tool (FAST). One sentence doesn't seem to do justice to this topic. As a reader who is only vaguely familiar with this tool, I found myself interested in how FAST has been used by FS biologists and what impacts, if any, it has had on project planning.
14. Pg. 143, in "Summary of stressors….regulatory mechanisms". I couldn't agree more with the following key statement: "There are few places in the analysis area where forest management practices are explicitly applied to conserve or benefit fishers".
15. Pg. 146, 3rd parag. Buskirk et al. (2012) also, and more recently, found that "Female survival has been shown to be the most important single demographic parameter…."

Comments on the Proposed Rule:

Much of what was summarized in the rule was laid out in more detail in the report, so I have not commented separately on the material in the rule. The one exception is the request to evaluate alternative spatial designations for the DPS. On this account, I prefer the original West Coast DPS boundaries. First, there is strong evidence for the historical distribution of fishers in significant portions of WA and OR, thus an effort to conserve the species should not exclude areas where their return via management is scientifically justified. Excluding much of OR and WA (as in Alt. #1) or the currently unoccupied area in the Sierra (as in Alt. #2) seems contrary to the goal of restoring the species to its historical range. Second, I don't believe that the genetic evidence is strong enough – based on a statistical differences in neutral markers and in the absence of samples from the currently unoccupied area – to justify splitting the DPS in California (Alt #2). In sum, a robust recovery strategy would consider the entirety of the historical range in the Pacific states.